

Interactive comment on “Tropical biomass burning smoke plume size, shape, reflectance, and age based on 2001–2009 MISR imagery of Borneo” by C. S. Zender et al.

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Reviewer 1 (Jeffrey Reid):

We are grateful for Dr. Reid's careful review. We modified the text of the manuscript to address most of Dr. Reid's suggestions. These suggestions have considerably strengthened the manuscript.

Detailed response (in plain text) interspersed with Reviewer 1 Comments (*in italics*):

In this paper the Zender et al. team used a MISR plume mapping tool to study the

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dimensions and retrieved properties of individual smoke plumes in Borneo. I think the actual measurement of plume physical dimensions is quite good. The MISR team has put a lot of effort into that tool. If the paper stuck to this, I would have few issues, except perhaps the issue of proper descriptions of sampling and sampling bias.

Thank you

In this regard, the authors briefly acknowledge but by no means emphasize the implications of sampling bias inherent in the measurement. This occurs at two scale. First, the 1030AM overpass of MISR implicitly favors morning fires, which are the vast minority. We have known for some time that there is a strong diurnal cycle in burning. For this particular region, I cite my own ACPD paper where we are finishing the review process. Jukka Miettinen's papers clearly show that in El Niño years, it is peat fires which are burning for days at a time which dominate emissions. Also, (although I don't hold these authors beholden to this), Ed Hyer has a paper in press which looks at MTSAT geo data and he found mammoth differences based on fuel type. So for this paper, we expect “long plumes” in the morning because it is peat fires, burning overnight, which likely dominate the sample. If this is the case, then I expect their sampling to be el niño dominated. So in regards to this paper, these caveats need to be fully discussed.

Agreed. Our discussion of the length discrepancy between Borneo and Central American plumes has been revised to elevate the role of sampling and the interaction between overflight-time bias and the diurnal and interannual cycle of Borneo fires. Additionally, we remind the reader in the conclusions that our estimate of the mean length of Borneo smoke plumes may not be representative of all Borneo plumes.

What fraction of the 300 fires were from El Niño years?

83% were from the dry, El Niño years of 2002, 2004, 2006, and 2009.

How do plumes differ based on fuel source (was it peat, ag, or other deforestation?).

73% of plumes in Borneo are from peatlands, or “Peat Forests” which Tosca et al. 2011

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defined as peat, freshwater swamp, and heath forest ecoregions (all of which have a peat soil) based on WWF maps. This is, coincidentally, the same as the fraction of plumes in Sumatra that originate from peatlands.

Where on the island where they measured?

Mostly in Central Kalimantan province, with lesser amounts in West and East Kalimantan as shown in Tosca et al., 2011 figure 2, and to Reid et al., 2012 figure 2a. Though differences in plume geometry may persist between plumes from different fuel sources, Table 1 in Tosca et al., 2011 shows minor differences between plumes heights in peat vs. non-peat forests.

How does this jive with the 1030 overpass time?

Most plumes in the database are from peatlands (73%), and were measured in El Nino years (83%). The two are not independent, since the fraction of fire pixels in peatlands increases in El Nino years. (Though, the fraction of plumes in peatlands actually decreases somewhat during El Nino years leading us to think that peatland fires and subsequently plumes during El Nino were larger.)

Tosca et al., 2011 show this in their Table 1 and 2, Figure S.2 and discuss it in their Section 3.1.

And perhaps most importantly, how should modelers use this information?

We address this question in Section 5 of the revised manuscript:

“Potential users should be aware of the limitations and biases in the parameterizations we develop based on MISR imagery. Our work incorporates all plumes identified in Borneo during 2001-2009. This period includes substantial interannual variability, such as four dry (El Nino) and five wet years. The plume shape, size, and age PDFs we derive are most characteristic of plumes from peatlands during dry years, and are based only on plumes sampled during MISR’s late-morning overflight time. Given the strong afternoon peak in the diurnal cycle of Borneo fire counts, our database may preferen-

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tially represent older, and thus longer, plumes. Sub-sampling our database for other fuel types, or meteorological conditions (e.g., wet years) would lead to different PDFs. However, this initial work gives researchers the first-order geometric and lifecycle characteristics of the plumes seen on Borneo by MISR. Mesoscale meteorological models can use this information to prescribe plumes with these empirical PDFs as a statistically accurate alternative to the more difficult task of predicting the shapes and ages of Borneo-like plumes.”

How does this fully relate to Tosca’s plume work?

Tosca et al., 2011 created and used the same plume database as a starting point to describe seasonal, interannual, climatological, and geographic characteristics of the plumes. The current manuscript focuses on the PDFs (rather than the mean) of the plume geometry and age. The same sampling issues present here are in that. Tosca et al., 2010 is primarily a model sensitivity study to the gross effects of all Indonesian smoke averaged up to the GCM scale, and has little to do with the size, shape, and optical properties of individual plumes that are the focus of the present manuscript.

These are all fixes which can be done in a month. Where I think the paper is on VERY shaky ground is on the optical retrievals and the implications for smoke particle evolution. It will take considerable effort to fix this part, and I am not even sure it is even doable with the data they have access to. At NRL we specialize in error matrices for satellite products, and optical retrievals on plume structures is a very hard gig. The short of it is that first and foremost, retrievals fail for very high AOTs. Essentially eventually scattering becomes semi-infinite and absorption dominates on AOT. So, people often use retrievals on the plume edge (such as here). But, for this intermediate range where even retrievals are possible, multiple scattering dominates and hence errors grow rapidly. For high AOTs, MISR underestimates by a large fraction, while MODIS overestimates. Implicit in this is that the optical models are not correct.

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Agreed. The original manuscript mentioned the uncertainty in all the optical properties, and the retrieved AOT limit of 2.5, but it did not discuss the underlying physics or technical issues that cause this limit, nor the biases expected to be revealed by more careful coeval analysis by independent instruments. The revised manuscript attempts to summarize the additional caveats raised by the Reviewer regarding the optical properties retrieved by MISR:

“Neither optical depth nor SSA have valid retrievals in regions of high opacity or cloud contamination (Nelson, et al., 2009), resulting in a maximum reported optical depth of 2.5. According to J. Reid (personal communication, 2012), MISR underestimates high AOTs by a large fraction, while MODIS overestimates them. Moreover, retrieval errors are likely to be large in regions of transition from high to low AOT (such as plume edges) where multiple scattering effects are important. We return to this point in Section 3.2.”

“Insofar as the halos mark the edges of plumes, they are located in regions of transition from relatively high to low optical depth. In such regions the effects of multiple scattering on retrievals is more pronounced (J. Reid, personal communication, 2012), and likely to contaminate the retrieved optical properties.”

For MISR in particular, it was my understanding that there was not much faith in the wo product by the developer. They may feel more comfortable in a relative sense,

Agreed. This is our understanding as well. The use of the relative optical properties was generally accepted (at the December MISR Data Users symposium) as the preferred method to exploit this valuable dataset without overinterpreting it.

but I personally have my doubts. Looking at radiance is better (which the current paper does), but they need do the RT to interpret what changes mean. I do not think they could do an adequate job on this in a month unless they brought it folks from the MISR team. Even then I am a bit dubious it could be salvaged in a month.

Respectfully, we disagree that the optical retrievals presented in this work, for our pur-

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poses, need (or merit) additional processing, inversion, or RT methods prior to publication. Our original manuscript clearly states that “the MISR science team strongly cautions against detailed analysis of top-of-atmosphere albedo data”, in agreement with the Reviewer. We always present the relative optical properties in addition to the absolute optical properties because the former are less sensitive to absolute calibration errors than the latter. The Reviewer seems to assert that all the optical properties (i.e., including AOT and single scatter albedo) retrieved by MISR are highly problematic and unusable, even when analyzed in the relative sense, as a basis to describe plume optical evolution. We wish the Reviewer had heard (and, if he did hear, then participate in) the discussion on this point at the MISR group science meeting in December when Zender presented this paper to a large audience of MISR scientists/users who are familiar with this issue. After some discussion (led mostly by Ralph Kahn), there was wide agreement that showing relative optical properties was an adequate, and even preferred, way to characterize spatial changes in optical properties with high retrieval uncertainties. Furthermore, the qualitative trends observed in our results do not change when we consider a 67-plume subset of the database that contains only those plumes that have no missing values. Plumes lacking missing values have lower AOTs and less cloud contamination, and thus are less likely to be affected by the biases the Reviewer identifies. The fact that they show similar trends indicates that the trends we see may be indicative of physical effects rather than sampling and retrieval biases.

The authors need to be a bit careful about the implications of their findings and I think they misunderstand the nature of smoke particle evolution. For example, they state that wo becomes 3% greater downwind. So even if this is true, the implication of wo going from 96%-99% is that secondary mass formation increases by a factor of three (even generously accounting generously for coagulative increase in the mass scattering efficiency)! The Carnegie Mellon guys get this in the lab for very fresh smoke, but this has not been directly observed in the environment. I for one found 40% increase from the top of the plume to downwind.

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It is unclear to us what quantity is referred to by the “40% increase” that the Reviewer found.

So this would be quite extraordinary. That does not at all mean that it is not happening, but evidence is tenuous. It could be increases in hygroscopicity as well correlated with inorganic like SO_4 (which has been pretty well shown now in the lab by Gavin McMeeking's-CSU work), and we think that SO_2 - SO_4 conversion is very rapid the peat plumes.

Agreed. The revised manuscript clarifies that inorganics, especially sulfate, likely play a role in increasing the hygroscopicity of peatland fires: “Mass extinction efficiency may also increase down-plume due to hygroscopic growth and aging, i.e., coatings and condensation of organic and inorganic species (e.g., sulfates)” and “Peat fires have a high ratio of smoldering to flaming combustion (Christian et al., 2003), and may contain significant amounts of sulfur which can produce bright, hygroscopic sulfate aerosol after combustion (Eck et al., 2003).”

Further, there findings on AOT, ω and radiance are incompatible with a 15-25% increase in AOT in along plume direction. Based on their findings, it should be much higher. Again, because AOTs cannot be retrieved at high values, what they are likely looking at is how diffuse the aerosol boundary is at the edges. This sampling bias is exactly the same as why you see peaks in AOT off the coast of California away from the source. But, again, I would suggest them working closely with the MISR team on these issues.

The revised manuscript clarifies that retrieval biases expected for large AOT:

“Relative optical depth (i.e., ratio of optical depth to maximum optical depth) shows a more marked change, increasing from 0.6 near the origin to 0.8 at the plume's end (Fig. 6d). Recall that optical depths greater than 2.5 are not retrieved, so low values near the head of the plume may actually indicate a greater fraction of high (and thus not-retrieved) values.”

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That said, the manuscript remains clear that the downplume increase is seen even in plumes with no missing values, i.e., in plumes where all AOT retrievals are “valid” and thus less than 2.5. Finally, we emphasize that we are simply reporting the statistics (e.g., averages) of MISR retrievals. MISR retrievals of AOT, SSA, and reflectance, may not always be self-consistent, due to biases such as the Reviewer notes. The reported values represent the best publicly available retrievals created by the MISR team over the course of many years of effort and attempting to improve (rather than report and interpret) these retrievals would go well beyond the scope of this paper.

The paper in many locations often places clauses and adjectives on the nature of fire and smoke evolution which sort of take the field for granted. In many cases, they cite my 2005 papers incorrectly. For example, I am cited as saying that there are only three fuel types (page 30992-In 10), then they list in the paper 4.

We agree that our descriptions (adjectives, clauses etc.) of fire and smoke in the original manuscript were looser and less consistent and nuanced than they should have been. And the Reviewer is the best judge of whether we have appropriately cited his papers. We appreciate the Reviewer's efforts to correct our inconsistencies and misinterpretations. We have endeavored to tighten the language in the revised manuscript. For example, the revised manuscript uses “reflectance” instead of “albedo” or “top of atmosphere albedo”.

We disagree on the Reviewer's specific example. First, we never say, or quote the Reviewer as saying, that “there are only three fuel types”. On p. 30992 line 10 we cite the Reviewer after stating that smoke plumes may be classified as originating in “tropical fires, savanna wildfires, and wildfires in temperate and boreal forests”. After which we explain why we do not count (in this study) smoke generated in pyrocumulonimbus events. The three broad sources classification seems to be a natural, high-level, taxonomy relevant to our paper. Nearly all of the outdoor entries in Reid et al. 2005a Tables 2 and 6 fall into these categories. Same for Tables 3, 4, and 5 of Reid et al. 2005b (Table 5 of which is based on exactly the three “broad biomes” we adopted).

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Of course fires could also be categorized based on geographic regions (as in Reid et al., 2012), based on finer ecological zones (like Tables 6 and 7 of Reid et al. 2005a, which segregate woody savanna and cerrado from grasslands/savanna), or based on more detailed fuel types. Our categorization is not meant to be exhaustive. The Reid et al., 2005a paper was cited as showing fires “may be divided in three broad categories” which seemed entirely consistent with the work of the Reviewer and others who often group fire properties (occurrence, C emissions, etc.) by their origin tropical forests, grasslands/savannas, or temperate/boreal forests. Since it appears that the Reviewer takes exception to this classification (or he is really saying that we cannot count past 3?), the revised manuscript omits that citation to his previous work. We also clarify that we are not counting or considering plumes associated with pyrocumulonimbus events. The overall classification is retained because it is widespread and it introduces the reader to terminology employed later in the manuscript.

What I did was point out that this is where there are measurements of physical properties, but I did not go comprehensively into fuel types. I bring up (section 8) other fuels like peat, but requires future study (which it does, hence 7SEAS and SEAC4RS).

Again, our broad categorization into three types is not meant to be comprehensive or detailed, and nowhere did/do we claim (or intend to imply) that the Reviewer says there are only 3 fuel types.

I am also quoted as saying AOTs are higher in Brazil than Africa (even here inappropriately cited to part 2, when really should be part 3). But regardless, I never said that. I listed a mean AOT from sun photometer in Table 3, but in Africa Mongu is outside of the main plume (I assume this is where they got it).

Agreed. The relevant citation has been corrected, and the reference to Zambian AOT removed.

On page 31009, in 15-22, they say that smoldering produces a higher fraction of hygroscopic organic to hydrophobic black carbon. Even here one must be careful. First,

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smoldering combustion produces no black carbon at all, so what black carbon is there is from flaming on the periphery. Second, particles tend to be internally mixed. Even from flaming combustion where one can get pretty much BC dominated aerosol particles, Martins showed that these can get coated with secondary organics pretty quickly.

The revised manuscript clarifies this, and explains that smoldering fires contain some flaming combustion. It also emphasizes the most important point of this paragraph: that smoldering fires have higher SSAs than flaming fires:

“Note that “smoldering fires” are largely but not exclusively fueled by smoldering combustion, since most fires contain a mixture of smoldering and flaming combustion. Smoldering combustion itself produces no black carbon; hence, smoldering fires have a greater ratio of bright organic carbon aerosols to dark black carbon aerosols (Reid et al., 2005b, Table 2; Christian et al., 2003), which originate in peripheral flaming combustion (J. Reid, personal communication, 2012). However, organics can quickly coat black carbon aerosols, resulting in more hygroscopic internally mixed particles (Martins et al., 1998). Overall, observations of smoldering fires have generally yielded much higher SSAs than observations of flaming fires.”

Regardless of the exact mechanisms that cause smoke from smoldering fires to be brighter than smoke from flaming fires (less BC? more POA emission? SOA formation? different coatings on BC?), we can say that higher SSAs are more consistent with smoldering fires than flaming combustion.

Finally, it is implied here that primary organics are hygroscopic. I guess they are if you think of f(80%) as 15% as hygroscopic.

The revised manuscript is more strict in its usage of “hygroscopic”. We must point out the potential role of hygroscopicity, but definitive statements are impossible without in situ data. As your 2005 paper summarizes, Gras et al. (1999) found $f(80\%)$ of Indonesian smoke was 1.5-2.2.

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There are too many cases of misunderstandings in the paper to list here, and I want to avoid ranting.

We have fixed all that the Reviewer has pointed out, and all that we could find ourselves.

Regardless, the chemistry is relatively complex and I think a re-review of the currently literature would be helpful to the authors.

Agreed. In our opinion, the manuscript deserves credit for assembling a nine-year database of all plume optical properties of biomass burning plumes retrieved by state-of-the-art sensors over an important region of fire emissions. Nowhere, to our knowledge, has the climatology of these properties been examined at the 18-km scale that MISR makes possible. The ensemble of these properties has yielded new insights, and confirmation of old insights, into the average behavior of plume optical properties. Yes, the retrievals are imperfect, as is the understanding and writing of the authors. Nevertheless, the omission or neglect of the best MISR optical retrievals currently available in favor of developing more complex retrieval schemes than the MISR science team itself has done, seems to us to be “throwing out the baby with the bathwater”. Much can be, and has been, learned from the analysis of these properties. And that includes improvements due to feedback from the Reviewers. We think the straightforward presentation of the MISR retrievals (in both relative and absolute format), along with appropriate caveats, is an appropriate next step that advances the community’s research into biomass burning.

The motivation of the paper is even a bit questionable. In the previous modeling paper in ACP paper by Tosca, I as a reviewer pointed out that the whole climate simulation was unphysical, but was worth publishing as a baseline. At the MISR user’s conference a month ago, I met Tosca and we agreed on this point. Indeed, adding el nino emissions to non el nino years ignores the covariences in the large scale meteorology of the region. Plus, a 10% decrease in precip is not what I would call significant in this case (I am not sure we could even measure precip to 10% in the region). Now, this model

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result is presented by these same authors as pretty much fact in the abstract and throughout the paper. This seems to be a repeated offense by modelers in general. So, this leads me in the future to not cut modelers much slack as I have in the past. It would help if discussions are placed in proper context.

The revised manuscript removed (from the abstract) and weakened (in the Introduction) the conclusions from Tosca et al., 2010 that the Reviewer objects to as overconfident or based on unrealistic modeling. Though some of those conclusions are undoubtedly true (biomass burning increases atmospheric absorption), others (precipitation feedbacks) were, as the Reviewer states, presented as facts rather than as tentative, model-dependent conclusions in need of further and more robust study. In any case, the present manuscript does not depend on the modeling results of Tosca et al., 2010. That said, we continue to fully endorse the conclusions of Tosca et al., 2010 that were, in fact, quite motivational for this manuscript. Aerosol-induced surface cooling and weakening of the hydrologic cycle (via reduced surface evaporation) are robust results seen in many studies besides our own.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 30989, 2011.

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